# The University of Melbourne

Carlton, N.3, 15th July, 1954.
VICTORIA, AUSTRALIA

Professor J. Lederberg,
Department of Genetics,
University of Wisconsin,
Madison 6,
WISCONSIN, U.S.A.

Dear Josh,

Many thanks for your letter of July 4th and your generous comments about my report. I am glad that I did put in the effort during my last week in Madison to write this report, first for you, but also for myself, because since returning it has been an invaluable piece of lab equipment and my assistant is consistently thumbing the pages for details of techniques, etc.

During the last month I have been spending as much time as I can in the lab and exclusively devoting my efforts to the yeast project. We have cultured most of the dried ampoules which Helen had prepared and they are viable and running true to form with a few minor hitches which can be resolved. I have not yet started any fractionation of the yeast, and am still waiting for our disintegrator to be made. I purchased a Hartridge Reversion Spectroscope which is working beautifully. My lab bench is like a replica painting of my bench at Madison with media and cultures piling up.

In regard to the eventual challenge which I must meet in looking for restoration I thought that the following general scheme might be more profitable than the proposals indicated in my report, namely this: We isolate from a donor auxotroph fraction X containing plasmids. This is incorporated into media inoculated with two petite auxotrophs of opposite mating types, but carrying different biochemical markers from the donor auxotroph. The mating would result in a prototroph which can be selectively isolated on LR medium and if the plasmids are taken up during the mating process then the resultant culture should be restored. The prototroph diploid can be further examined by an inducing sporulation and determining the segregation in the ascospores. Since This approach has an advantage over a frontal attack of trying to transduce a normal plasmid into a growing petite, at the time of fusion of the opposite mating types is there may be a good chance of the plasmids being incorporated into the cytoplasm of the developing diploid. Furthermore, by selection of the appropriate auxotrophs we can effectively

direct the experiments to exclude donor and petite contamination. Finally, if the system did work there would be an intriguing follow-up in the segregation of the ascospores. I will not abandon, of course, the other methods which I suggested, but I would value your comments on the above proposal. If it is sound I don't think we should discuss it widely until I have put the proposal to a preliminary experimental test.

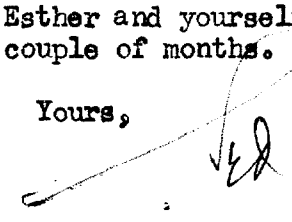
I had a letter from Hans who told me that he was taking some of the embedded yeasts to Stockholm. He sent me some nice photo-micrographs of sections prepared in Madison.

I would like to have cultures of Caroline Raut haploids as I may have to prepare some auxotrophs to meet the requirements of the mating-restoration experiments. The TM2 phage and chemicals also arrived intact, for which many thanks. Once the yeast programme gets beyond the budding stage I think I might have a look at the streptomycin resistance transduction. Dr. Gibson who has been doing some interesting work on streptomycin uptake by sensitive and resistant cells finds, like you did, that the SR cells respond to aeration as do SS. Your work on the coli recombination sounds intriguing and I have no doubt it won't be long before you show a conjugation tube between cells.

I enjoyed the tit-bits of news and about the movements of Tom and Boris in and out of Madison. Am so pleased young Bernard received my letter from Pitcairn. Michael is about to write to him.

My best wishes to Esther and yourself. You shall hear from me again during the next couple of months.

Yours,



P.S. Congratulations on your picking the few typing errors which crept into the report. 'Coon' was Mrs. Laver's interpretation of my writing of 'cross'. I think she did a magnificent job under very difficult circumstances.